These orbits are in very satisfactory confirmation of each other.

May we hope that at no distant period Dr. Doberck may find he has sufficient material to induce him to investigate the elements of α Centauri; a fair approximation to the true orbit might be expected from his experienced hand.

PROF. ADAMS ON LEVERRIER'S PLANETARY THEORIES[1]

II.

THE nineteenth chapter of M. Leverrier's researches, which forms the first part of the eleventh volume of the *Annals of the Paris Observatory*, contains the determination of the secular variations of the elements of the orbits of the four planets, Jupiter, Saturn, Uranus, and Neptune.

In the first place are collected the differential formulæ which are established in the previous chapter, and which give the rates of secular change of the various elements at any epoch in terms of the elements themselves, which by the previous operations have been cleared of all periodic inequalities.

The terms of different orders which enter into these formulæ are carefully distinguished.

If we were to confine our attention to the terms of the first degree with respect to the excentricities and inclinations of the orbits, and of the first order with respect to the masses, the differential equations which determine the secular variations would become linear, and their general integrals might be found, so as to give the values of the several elements for an indefinite period.

In the present case, however, the terms of higher orders are far too important to be neglected, and when these are taken into account the equations become so complicated as to render it hopeless to attempt to determine their general integrals.

Fortunately, however, these are not needed for the actual requirements of astronomy, and for any definite period the simultaneous integrals may be determined with any degree of accuracy that may be desired by the method of quadratures.

In this way M. Leverrier has determined the values of the elements for a period of 2,000 years, starting from 1850, at successive intervals of 500 years. The first steps in this integration were attended with some difficulties, because the determination of the numerical values of the rates of change of the several elements at the various epochs depends on the elements themselves which are to be determined. Hence several approximations were necessary in order to obtain the requisite precision.

After this work of M. Leverrier, however, the extension of the investigation to other epochs, past or future, is no longer attended with the same difficulties. In fact, from his results we may at once find, by the method of differences, very approximate values of the elements at an epoch 500 years earlier or later than those which he has considered. His general formulæ will then give the rates of change of the several elements at the epoch in question, and having these we can determine by a direct calculation the small corrections which should be applied to the approximate values of the elements first found.

This process may evidently be repeated as often as we choose.

It is important to remark that in the formulæ which give the rates of change of each of the elements at the five principal epochs considered, as well as in those which give the total variations of the elements at the same epochs, the masses of the several planets appear in an indeterminate form, so that it may be at once seen what part of the variation of any element is due to the action of each of the planets, and what changes would be produced in the value of any element at any epoch by any changes in the assumed values of the masses.

Consequently, when the astronomer of the future, say of 2,000 years hence, has determined the values of the elements of the planetary orbits corresponding to that epoch, it will be easy for him, by comparing those values with the general expressions given by M. Leverrier, to determine with the greatest precision the actual values of the masses, provided that all the disturbing bodies are known; and should there be any unknown disturbing causes, their existence would be indicated by the inconsistency of the values of the masses which would be found from the different equations of condition.

By means of the work which has just been described, everything has been prepared which is required for the treatment of the theories of the several planets.

The remainder of the eleventh volume of the *Annals* is accordingly occupied by the complete theories of Jupiter and Saturn, the former theory being given in Chapter 20, and the latter in Chapter 21 of M. Leverrier's researches.

The coefficients of the periodic inequalities of the mean longitudes and of the elements of the orbits are not only exhibited in a general form, but are also calculated numerically for the five principal epochs considered in Chapter 19 of these researches, viz., for 1850, 2350, 2850, 3350, and 3850.

The long inequalities of the second order with respect to the masses, depending on twice the mean motion of Jupiter plus three times the mean motion of Uranus minus six times the mean motion of Saturn, are also determined in a similar form.

Chapter 22 of M. Leverrier's researches, forming the first part of the 12th volume of the "Annals," contains the comparison of the theory of Jupiter with the observations, the deduction of the definitive corrections of the elements therefrom, and finally the resulting tables of the motion of Jupiter. The observations employed are the Greenwich observations from 1750 to 1830 and from 1836 to 1869 together with the Paris observations from 1837 to 1867.

To the results given in the Astronomer-Royal's "Reduction of the Greenwich Observations of Planets from 1750 to 1830," M. Leverrier has applied the corrections which he has found to be required by his own reduction of Bradley's observations of stars and his redetermination of the Right Ascensions of the fundamental stars, published in the second volume of the "Annals" (Chapter 10).

The equations of condition in longitude, for finding the corrections of the elements and of the assumed mass of Saturn, are divided into two series corresponding to the observations made from 1750 to 1830, and into two other series corresponding to the observations made from 1836 to 1869. Moreover in each, of these series the equations are subdivided into eight groups, corresponding to the distances of the planet from its perihelion, 0° to 45°, 45° to 90°, and so on. From these are formed four final equations, the solution of which gives the corrections of the epoch, of the mean motion, of the excentricity, and of the longitude of the perihelion, in terms of the correction required by the mass of Saturn, which is left in an indeterminate form. The substitution of these expressions in the thirty-two normal equations corresponding to the several groups above-mentioned, gives the residual differences between theory and observation in terms of the correction of the mass of Saturn. No conclusion can be drawn from the ancient observations; but from the modern observations M. Leverrier finds that the mass of Saturn assumed—which is that of Bouvard—should be diminished by about its $\frac{1}{2\,0\,0}$th part. This correction is very small, but M. Leverrier regards it as well established.

On the other hand, Bessel's value of the mass of Saturn, founded on his observations of the Huyghenian satellite, exceeds Bouvard's by about its $\frac{1}{2\,6\,0}$th part.

[1] Continued from p. 464.

The equations of condition in latitude are treated in a similar manner, being grouped according to the distances of the planet from its ascending node. From these equations the corrections of the inclination of the orbit and longitude of the node are found separately from the ancient and from the modern observations. The results differ very little, but the second solution is employed in the construction of the tables. After the application of these corrections to the elements, the agreement between theory and observation may be considered perfect; so that the action of the minor planets on Jupiter appears to be insensible, and there is no indication of any unknown disturbing causes.

There are some peculiarities in the mode of tabulating the perturbations caused by the action of Saturn. The perturbations of longitude and of radius vector are not, as usual, exhibited directly, but instead of them M. Leverrier gives the perturbations, both secular and periodic, of the mean longitude, of the longitude of the perihelion, of the excentricity, and of the semi-axis major of the orbit, and then from the elements corrected by these perturbations he derives the disturbed longitude and radius vector by the ordinary formulæ of elliptic motion.

Where the perturbations are large M. Leverrier considers this preferable to the ordinary method of proceeding. The perturbations of latitude being small, he applies to the inclination and longitude of the node their secular variations alone, and then determines directly the periodic inequalities of latitude.

All these perturbations, whether of the elements or of the latitude, are developed in a series of sines and cosines of multiples of the mean longitude of Saturn, including a constant term, the coefficients multiplying these several terms being functions of the mean elongation of Saturn from Jupiter, which for a given elongation are developed in powers of the time reckoned from the epoch 1850. These coefficients only are tabulated with the mean elongation as the argument, and the perturbations are thence calculated by means of the ordinary trigonometrical tables. The intervals of the argument are so small, that the requisite interpolations are very simple, and the coefficients which relate to the four elements, and depend on the same argument, are given at the same opening of the tables.

The tables have been calculated specially for the 500 years included between the years 1850 and 2350. Nevertheless they may be applied to epochs anterior to 1850, by simply changing the sign of the time reckoned from 1850. For one or two centuries before 1850 this extension will have all the rigour of modern observations, while for still earlier times the accuracy of the tables will greatly surpass that of the observations which we have to compare with them.

M. Leverrier's Tables of Jupiter are now employed in the computations of the *Nautical Almanac*, beginning with the year 1878.

The thirteenth volume of the *Annals* is devoted to the theories of Uranus and Neptune. These theories are not unattended with difficulties. In the first place, these planets are disturbed by the actions of the two great masses Jupiter and Saturn, interior to their orbits, and these actions are modified by the great inequalities of Jupiter and Saturn depending on five times the mean motion of Saturn minus twice the mean motion of Jupiter. In the next place twice the mean motion of Neptune differs very little from the mean motion of Uranus, and thus arise inequalities of long period in the elements of their orbits which are large enough to produce very sensible terms of the second order. Lastly, the mean elliptic elements of the two planets are not yet sufficiently well known.

In a preliminary chapter, the 24th, M. Leverrier investigates formulæ which are specially applicable to the case of a planet disturbed by another which is considerably nearer to the sun. In this case it is easily seen that, by the direct action of the disturbing planet on the sun, perturbations of large amount may be produced in the *elements* of the orbit of the disturbed planet, while the corresponding perturbations of the co-ordinates of the planet are comparatively small. Hence arises the advantage of considering this case apart.

We have seen how closely the theories of Jupiter and Saturn are related to each other. In a similar manner the theories of Uranus and Neptune are also closely related in consequence of the great perturbations introduced into the elements of their orbits by the near approach to commensurability in their mean motions. Hence, before entering upon the separate theories, M. Leverrier devotes Chapter 25 of his researches to the determination of the mutual actions of Uranus and Neptune, and this forms the base of the theories of both planets. The method employed is similar to that adopted in the case of Jupiter and Saturn, and the results are exhibited in the same general form.

It is important to remark that the elements of Uranus and Neptune as determined from observations severally differ from their mean elliptic values by the amount of their perturbations of long period corresponding to the mean epoch of the observations. The apparent elements of Uranus and Neptune for the epoch 1850 have been carefully determined by Prof. Newcomb in his excellent work on the theory of those planets which obtained the Society's medal in 1874. By the application of his own general formulæ, M. Leverrier deduces from these elements the values of the mean elliptic elements corresponding to the same epoch. It may be remarked that the mean elements thus determined will depend on the assumed masses of the two planets, and will therefore require small corrections when more accurate values of the masses have been obtained.

When the secular variations of Uranus and Neptune given in Chapter 19 were found, the elements were less accurately known, and M. Leverrier has therefore recalculated the values of the excentricities and longitudes of the perihelia of the two planets for the same five epochs as before, starting from the mean elliptic values of the elements above referred to.

Chapter 26 contains the completion of the theory of Uranus. The last chapter, which contains the completion of the theory of Neptune, is not yet printed.

The twenty-third chapter also, which contains the comparison of the theory of Saturn with observations, together with the tables of the planet, and which will form the latter part of the twelfth volume of the *Annals*, is not yet printed. The results of this comparison of the theory with observations have, however, been fully published in the *Comptes Rendus*, and I understand that the tables will be used for computing the place of Saturn in the forthcoming volume of the *Nautical Almanac*.

Although the comparison of the theory of Saturn with observations shows in general a satisfactory accordance, there occur some discrepancies in individual years which are larger than might be desired.

During the thirty-two years over which the modern observations extend, viz., from 1837 to 1869, the discrepancy between theory and observation, however, remains constantly less than 2″·5 of arc, excepting in two instances, viz., in the years 1839 and 1844, when the differences amount to 4″·5 of arc.

In the ancient observations only, made in the time of Maskelyne, rather larger differences occur, amounting in two instances to nearly 9″ of arc.

In order to test whether these discrepancies could be due to any imperfections in the theory, M. Leverrier has not shrunk from the immense labour of forming a second theory of the planet independent of the former, employing methods of interpolation instead of the analytical developments. I learn directly from M. Leverrier that this

second investigation entirely confirms the accuracy of the first as regards the periodic inequalities, but that the secular variations of the excentricity and longitude of the perihelion are slightly changed. The effect of these changes is to bring the theory into very satisfactory accordance with the observations of Bradley, but the discrepancies above mentioned in the time of Maskelyne and in the modern observations still remain unaffected. The character of the discrepancies shown by the modern observations makes it very improbable that they can be due to any errors in the theory.

In fact, the error appears to change almost suddenly from a positive one of 4″·4 in 1839 to a negative one of 5″·0 in 1844, a variation of nearly 9″·5 in five years. Now no terms or group of terms due to the action of the planets could thus suddenly disturb the motion in five years, at a given epoch, and then leave the motion unaffected during the following twenty-five years. M. Leverrier is therefore inclined to think that the discrepancies arise from errors in the observations, notwithstanding that the Greenwich and Paris observations are mutually confirmatory of each other.

He suggests that it is possible that the varying aspects presented at different times by the ring may affect the accuracy of the observations of the planet, and may cause changes in the personal equations of the observers, which, from being rather large in the case of the ancient observations, have gone on diminishing as the system of observation has become more perfect.

One unlooked-for result follows from M. Leverrier's comparison of his theory of Saturn with the observations. Considering that the influence of Jupiter on the longitude of Saturn may amount to 3800″, it might have been expected that from observations of the planet extending over 120 years the mass of Jupiter could have been determined with great precision. M. Leverrier has found, however, that this is not the case.

The equations of condition furnished by the comparison of the heliocentric longitudes of Saturn as deduced from theory and observation contain five unknown quantities, viz., the corrections of the assumed values of four elements and the correction of the assumed mass of Jupiter. On solving the equations with respect to the first four unknown quantities, the corrections to be applied to the elements are found to be greatly influenced by the intermediate correction of the mass of Jupiter, and after they have been substituted in the equations of condition, the coefficients of the correction of the mass of Jupiter in great part destroy each other, nowhere amounting in the resulting equations to one-tenth part of their values in the primitive equations. Hence these equations are insufficient to determine the mass of Jupiter with any precision. Consequently, in the formation of the Tables of Saturn, M. Leverrier has employed the value of the mass of Jupiter determined by the Astronomer-Royal from his observations of the 4th satellite.

The result which has just been noticed will appear to be less paradoxical if we consider that by far the larger part of the disturbances which Jupiter produces in the motion of Saturn is represented by the inequalities of long period which affect the mean longitude and the elements of the orbit. Now in the course of 120 years these inequalities have run through only a small part of their whole period, and therefore, during this interval, the greater part of their effects may be represented by applying changes to the several mean elements equal to the mean value of the corresponding long inequalities during the interval. It is only from the residual disturbances, which are comparatively small in amount, that any data can be obtained for the correction of the mass of Jupiter.

In the course of a few centuries, when these long inequalities, as well as the secular variations of the elements of Saturn, shall have had time to develop themselves, it will be possible to determine the mass of Jupiter from them with all desirable precision.

THE GLANDULAR ORIGIN OF CONTAGIOUS DISEASES [1]

TEN years ago, on the occasion of a Congress held in this town to discuss the question of the disposal of sewage, I had the honour, at the request of the committee of management, to deliver a lecture on the subject of the poisons of the spreading or communicable diseases. An abstract of the lecture was afterwards printed by the Congress, and for a time it gained a wide circulation.

The lecture of which I speak was based on a series of experimental researches which for some years previously I had been carrying out on the question of the mode of production and communication of those diseases which were anciently called plagues or pestilences, but which are now called communicable or spreading diseases.

I do not think that at a health congress like the present I can do better than recall attention to this same subject. The suppression of plagues is one of the grandest and supremest efforts of the sanitary reformer. The suppression can never be accomplished until all educated people understand the advances of modern science as to the cause and mode of origin and mode of propagation of these diseases. Whatever, therefore, tends to strike out light of knowledge on these subjects tends to elucidate, and though the spark lighted may go out again it may help to show the way.

I shall in this present effort first go back to the point where I stood when here ten years ago. I shall then briefly survey the course of thought that has sprung up between that time and the present. Next I shall state the position of my own views as influenced by the work of the past ten years. Lastly, I shall touch for a moment or two on the practical applications of theory to the development of practice.

Outline of the Glandular Theory.

From my researches previous to the year 1867, and which formed the subject-matter of my previous lecture here, I had discovered that the fluids secreted during various stages of disease in some forms of communicable disease could be made to propagate disease. A portion of secreted fluid taken from a patient of Mr. Spencer Wells, a patient who was suffering from surgical fever following upon the operation of ovariotomy, had been made to produce a definite form of fever in an inferior animal by being simply brought into contact with the peritoneal surface of the animal. The secretion from the peritoneum of the affected animal was shown by further experiments to have the power of inducing the same order of phenomena of disease in other similar animals, and through four generations of animals the phenomena were repeated. These were the first experiments in which this class of phenomena of disease by direct propagation and repropagation were produced synthetically. They have since been performed and modified in many ways, and the origination of them has been assigned to different experimentalists, but I am entitled to say they were the first of the kind; they were carried out in the years 1864-65, and they were communicated to the Association of Medical Officers of Health in the year 1865.

During the same course of research I made an attempt to separate the poisonous matter from the poisonous secretion, and in one attempt of the sort I believed myself to have been successful. Certainly I separated a substance which was exceedingly poisonous in its action, and which, after the manner of an alkaloid, combined with

[1] A Theory as to the Natural or Glandular Origin of the Contagious Diseases. Address by the president, Benjamin W. Richardson, M.D., F.R.S., at the Sanitary Congress, Leamington, October 3.